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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XLIII. Supplementary Number. No. 9.

Additional Note to Mr. Stone's Paper on the Change in the adopted Length of the "Tabular Mean Solar Day," which takes place with every Change in the adopted value of the Sun's Mean Sidereal Motion.

In the *Comptes Rendus*, No. 3 (July 16, 1883), M. Gaillot has inserted a paper in which he contests the accuracy of my results, with respect to the change of the unit of time with the change of the Sun's mean motion from n' to $n' + \delta n'$, upon two grounds:

(1) A mathematical investigation based upon the assumption that if T be the mean solar day, we can treat T as a function of n and n' considered as independent variables in the equation of condition

$$(n - n' - p + a \cos \omega - u)\,\mathrm{T} = 2\pi;$$

and

(2) From the small differences which he finds between a direct numerical calculation of the sidereal times at mean noon from Le Verrier's and Bessel's results. This is really a restatement in another form of Sir G. B. Airy's objection. The following is my answer to both these objections: I deny the mathematical legitimacy of M. Gaillot's assumption on page 152, line 10, "Les variables n', n et P sont indépendantes entre elles." I say that such an assumption is erroneous. For n is the "mouvement de rotation de la Terre autour de son axe," and n' is the "moyen mouvement apparent de révolution du Soleil," and they are therefore the numerical representatives of physical facts, and are directly proportional to the adopted unit of time. The ratio, therefore, of $n : n'$ is *constant and independent of the unit of time adopted.* I maintain, therefore, that the condition of constant proportionality between n and n' must be introduced into the equation of condition which fixes the unit of time. If I am right upon this

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point M. Gaillot's results are wrong; if M. Gaillot is right in assuming that n, n', and P also, can be treated as independent variables in fixing the length of the unit of time by the equation of condition which he has given, then my results are wrong, and M. Gaillot's deductions are correct.

M. Gaillot's numerical calculations prove nothing whatever with respect to the point at issue; for if LV and B represent the mean sidereal times, according to Le Verrier and Bessel, and

$$\mathrm{B} = \mathrm{C} + (p + n')t + 2\pi t$$
$$\mathrm{LV} = \mathrm{C}' + (p + n' + \delta n')t' + 2\pi t',$$

where t and t' are supposed to be expressed in the mean solar days of their respective scales.

Then, if $t = t'$, we have,

$$\mathrm{LV} - \mathrm{B} = \mathrm{C}' - \mathrm{C} + \delta n' t,$$

a result which will agree with M. Gaillot's numerical calculations.

But if the unit of time is different in LV on account of the change from n' to $n' + \delta n'$, as I maintain, then,

$$t = t'\left(1 + \frac{\delta n'}{n'}\right),$$

and therefore

$$\mathrm{B} = \mathrm{C} + (p + n' + \delta n')t' + 2\pi t' + (p + 2\pi)\frac{\delta n'}{n'} . t'$$
$$\mathrm{LV} = \mathrm{C}' + (p + n' + \delta n')t' + 2\pi t'$$
$$\therefore \mathrm{LV} - \mathrm{B} = \mathrm{C}' - \mathrm{C} - (p + 2\pi)\frac{\delta n'}{n'} . t'$$

The term $2\pi\frac{\delta n'}{n'}.t'$ is not small, but increases by about $1^{s}\cdot46$ per annum. It is to the neglect of this term on the erroneous assumption that we can have simultaneously $2\pi t$ equal to a multiple of 2π at every mean noon, whether we are using Bessel's or Le Verrier's expression for the sidereal time at mean noon, which has led, in my opinion, to serious errors in our present reckoning of mean solar time.

The question which I have raised is not, therefore, touched by such calculations as those of M. Gaillot, for in them it is *tacitly assumed* that the unit of time is not changed by a change in n', and this is the very point at issue. The question of the reality of the change in the unit of time by the change of n' to $n' + \delta n'$ can only be decided by theory or by a direct reference to the facts of observation. I have, in my opinion, proved the point theoretically, but I have also re-reduced the Lunar observations made at this Observatory and at Greenwich on the assumption of the correctness of my views, and I find that the errors of the Lunar tables from 1864 to 1882 are then very nearly, if not quite, as small as those which existed from 1847 to 1863, and a discussion of the occultations confirms this result.

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I might also mention that in the case of the N.P.D.'s, where the tabular quantities are sometimes rapidly increasing and sometimes rapidly decreasing, I find that with our present reckoning of time the tabular N.P.D.'s are systematically larger than the observations whenever the N.P.D. is rapidly increasing, and smaller than the observations whenever the N.P.D. is rapidly decreasing, and that these discrepancies are sensibly removed in my corrected results.

Determinations of Longitude on the East Coast of Africa. By W. H. Finlay, B.A., Chief Assistant, Royal Observatory, Cape of Good Hope.

It was decided in 1880 that a telegraphic determination of the difference of longitude between Aden and the Cape Observatory should be made, and I left the Cape on May 28, 1881, to take part in this operation.

The opportunity seemed an excellent one for determining also the longitudes of the various ports of call on the East Coast, and I determined to avail myself of it, at the suggestion and with the co-operation of the Astronomer Royal at the Cape.

I took with me an 8-inch sextant, by Dollond, an altazimuth by Troughton and Simms (which was also used for determinations of latitude at Durban and Aden), and three chronometers, viz. Parkinson and Bouts 801, Cribb 722 (both mean time), and Dent 1589 (sidereal).

The sidereal chronometer was always used for observing, and was always compared before and after observations with the others, which were kept while on board in my cabin, where the changes of temperature were not considerable.

The altazimuth was one of those used in the Transit of *Venus* Expedition, 1874, and a full description of it will be found on page 18 of the British Observations of the Transit. The values of the divisions of the spirit-levels were determined before I left the Cape, and at every observation the preceding and following divisions of the vertical circle were always read; so that the corrections of runs of the microscope micrometers at any place were always determined from the observations there; this correction, however, was very small. The sextant was in good adjustment. Observations at about the same altitude as those actually made during the Expedition were made before leaving, and showed that no sensible correction was required to the resulting local time for excentricity.

I arrived at Lourenço Marques (Delagoa Bay) on July 2, and by the kindness of the superintendent was allowed to take up my quarters at the station of the Eastern and South African Telegraph Company till the arrival of the British India Company's steamer, which was to take me on to Aden.